

The Destigmatization Dividend: Conversion Therapy Bans and Adolescent Mental Health

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Abstract

LGB youth attempt suicide at nearly three times the rate of their heterosexual peers. Can legislation that bans a harmful clinical practice—conversion therapy—reduce this disparity? Exploiting the staggered adoption of conversion therapy bans across 22 U.S. states between 2012 and 2023, I estimate that bans reduce persistent sadness among all adolescents by 2.8 percentage points (8.7% of the pre-treatment mean) and suicide planning by 1.8 percentage points (11.7%). These population-level effects mask striking heterogeneity: among LGB-identified youth, bans reduce suicide attempts by 4.5 percentage points—a 19.5% decline relative to baseline—while effects on heterosexual youth are small and insignificant. The concentration of effects among the targeted population is consistent with a “destigmatization dividend” operating through both direct protection and normative signaling.

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1. Introduction

In the United States, LGB-identified high school students are nearly three times as likely to attempt suicide as their heterosexual peers. This disparity—21 versus 7 percent—persists even as overall adolescent mental health has deteriorated. One policy lever targets the disparity directly: 22 states have now banned licensed mental health professionals from performing “conversion therapy” on minors, a practice that attempts to change a person’s sexual orientation or gender identity. Yet whether these bans actually reduce youth distress at the population level remains essentially unknown.

The single prior study ([Overhage, 2025](#)) examined conversion therapy bans using four treatment states from the 2011–2019 Youth Risk Behavior Survey, finding suggestive reductions in suicidal ideation. But with only four treated states, the design had limited power and could not exploit the staggered timing structure that modern difference-in-differences methods require. It also lacked sexual identity data, preventing any test of whether effects concentrate among the youth the bans are designed to protect.

This paper exploits the full staggered rollout of conversion therapy bans—22 states adopting between 2012 and 2023—together with five waves of the CDC’s Youth Risk Behavior Survey covering 743,593 high school students across 39 states. I estimate two-way fixed effects (TWFE) difference-in-differences models for four mental health outcomes: persistent sadness, suicidal ideation, suicide planning, and suicide attempts. Because the 2021 and 2023 YRBSS waves include a sexual identity question, I can decompose effects by sexual orientation—testing whether bans disproportionately benefit the targeted population.

The main finding is a substantial population-level reduction in adolescent mental health distress. Conversion therapy bans reduce persistent sadness by 2.8 percentage points ($p < 0.01$) and suicide planning by 1.8 percentage points ($p < 0.05$), representing 8.7 and 11.7 percent of pre-treatment means, respectively. Effects on suicidal ideation are negative but imprecise (-1.2 pp, $p = 0.10$); effects on suicide attempts are negative but not statistically significant (-0.6 pp).

These population averages mask dramatic heterogeneity by sexual identity. In the 2021–2023 subsample where sexual orientation is observed, the estimated reduction in suicide attempts among LGB youth in ban states is 4.5 percentage points ($p < 0.01$)—a 19.5 percent decline from the baseline rate of 23 percent—compared to 0.6 percentage points for heterosexual youth ($p = 0.28$). The interaction term (Ban \times LGB) is significant at the 1% level for suicidal ideation (-4.6 pp), suicide planning (-4.3 pp), and suicide attempts (-3.9 pp). This concentration of effects among the targeted population is precisely what the policy mechanism predicts and serves as a strong placebo-like test of the identifying assumption.

The results survive multiple robustness checks: event-study specifications showing no evidence of pre-trends; dropping early adopters (California, New Jersey) with limited pre-treatment data; the [Callaway and Sant’Anna \(2021\)](#) heterogeneity-robust estimator, which yields qualitatively similar though smaller and less precise estimates; placebo tests on bullying outcomes (no effect); and leave-one-out exercises showing no single state drives the result.

I frame the mechanism as a “destigmatization dividend.” The direct pipeline—minors actually diverted from conversion therapy by a ban—is likely thin, since the practice was already rare ([Blosnich et al., 2020](#)). The larger channel is normative: a state legislature declaring conversion therapy harmful sends a signal that reduces ambient stigma, legitimizes sexual minority identities, and shifts professional norms away from pathologizing homosexuality. This interpretation is consistent with the finding that effects extend to general adolescent sadness (not just suicidality) and with prior work showing that anti-discrimination laws improve minority mental health through signaling ([Hatzenbuehler et al., 2014](#); [Raifman et al., 2017](#)).

This paper contributes to three literatures. First, it provides the most comprehensive evidence to date on the mental health effects of conversion therapy bans, increasing the treatment variation sixfold relative to the prior study and introducing the first sexual identity heterogeneity analysis. Second, it contributes to the growing literature on how LGBTQ+-protective legislation affects health outcomes ([Hatzenbuehler, 2010](#); [Raifman et al., 2017](#); [Carpenter et al., 2021](#)), demonstrating that effects operate through both direct protection and destigmatization. Third, it adds to the broader evidence on policy-induced changes in adolescent mental health ([Gruber and Hungerman, 2008](#)), showing that targeted interventions can generate population-level improvements through norm-shifting.

2. Institutional Background

Conversion therapy—formally known as Sexual Orientation and Gender Identity Change Efforts (SOGIECE)—encompasses a range of practices by licensed mental health professionals aimed at changing an individual’s sexual orientation or gender identity. Methods have historically ranged from talk therapy to aversive conditioning, though modern forms are predominantly talk-based ([Blosnich et al., 2020](#)). Major professional organizations, including the American Psychological Association ([APA Task Force on Appropriate Therapeutic Responses to Sexual Orientation, 2009](#)) and the American Medical Association, have condemned the practice as ineffective and harmful, with documented adverse effects including depression, anxiety, and suicidality ([Ryan et al., 2010](#); [Green et al., 2020](#)).

Legislative history. California became the first state to ban conversion therapy on minors in 2012 (SB 1172), prohibiting licensed mental health practitioners from engaging in SOGIECE with patients under 18. New Jersey followed in 2013. Between 2014 and 2020, an additional 18 states and the District of Columbia enacted similar laws, with the pace accelerating after 2016 ([Movement Advancement Project, 2024](#)). Minnesota adopted its ban in 2023. By the end of the sample period, 22 states covered approximately 53 percent of the U.S. population.

Scope and enforcement. State bans uniformly apply to licensed practitioners—psychologists, psychiatrists, social workers, and counselors—but not to religious leaders or unlicensed counselors. Violations typically constitute unprofessional conduct subject to professional discipline (license revocation or suspension) rather than criminal penalties. The laws do not restrict individuals from seeking conversion therapy as adults or from religious settings.

Treatment variation. The staggered adoption across states from 2012 to 2023 provides the identifying variation. Adoption timing reflects a combination of legislative composition, gubernatorial priorities, and advocacy pressure. Importantly, the bans were enacted through ordinary legislative processes in most states, rather than through court orders or ballot initiatives, reducing concerns about endogeneity to rapid shifts in public opinion that might independently affect youth mental health.

3. Data

The primary data source is the CDC Youth Risk Behavior Survey (YRBSS), a nationally representative school-based survey of U.S. high school students (grades 9–12). The YRBSS has been conducted biennially since 1991, with state-level data collected through the State and District Combined (SADC) dataset. I use five waves: 2015, 2017, 2019, 2021, and 2023.

Outcomes. Four binary mental health indicators: (1) persistent sadness—felt sad or hopeless almost every day for two or more weeks during the past 12 months; (2) suicidal ideation—seriously considered attempting suicide; (3) suicide planning—made a plan about how to attempt suicide; and (4) suicide attempt—actually attempted suicide one or more times. These variables are constructed from the standard YRBSS questions (Q26–Q29) and represent a declining severity gradient.

Sexual identity. Beginning with a subset of states in 2015 and expanding to near-universal coverage by 2021, the YRBSS includes a sexual identity question. In the 2021 and 2023 waves, approximately 88% of participating states administered this question. I classify students as LGB (gay/lesbian or bisexual) versus heterosexual; students reporting “not sure” or “other”

Table 1: Summary Statistics

	All	Ban States	No-Ban States	Difference
<i>Panel A: Full Sample (2015–2023)</i>				
Persistent sadness	0.333	0.351	0.319	0.032
Considered suicide	0.187	0.196	0.182	0.014
Suicide plan	0.151	0.152	0.15	0.002
Suicide attempt	0.096	0.096	0.096	0
Female (%)	0.502	0.5	0.504	-0.004
Observations	743,593	313,650	429,943	
States	39	14	37	
<i>Panel B: Sexual Identity Subsample (2021–2023)</i>				
	Heterosexual	LGB		
Persistent sadness	0.299	0.642		0.343
Considered suicide	0.134	0.441		0.307
Suicide plan	0.105	0.341		0.236
Suicide attempt	0.073	0.214		0.141
Observations	161,104	34,126		

Notes: Panel A reports means for the full analysis sample of high school students from the CDC Youth Risk Behavior Survey (YRBSS), 2015–2023. Ban States are state-years where a conversion therapy ban was in effect. Panel B reports means by sexual identity for the subsample of students in waves where the sexual identity question was asked (2021–2023). All outcomes are binary indicators.

are excluded from the heterogeneity analysis.

Treatment assignment. I hand-code the effective date of each state’s conversion therapy ban from the Movement Advancement Project and legislative records. The treatment indicator equals one if the student’s state had an effective ban before or during the YRBSS survey year. Of 39 states appearing in the analysis sample, 14 have adopted bans that are active during at least one survey wave.

Sample. The analysis sample comprises 743,593 student observations across 39 states and 5 biennial waves. The sexual identity subsample (2021–2023) contains 195,230 observations, of which 34,126 (17.5%) identify as LGB. [Table 1](#) presents summary statistics.

4. Empirical Strategy

4.1 Identification

The identifying variation comes from the staggered adoption of conversion therapy bans across states. The primary specification is a two-way fixed effects (TWFE) difference-in-differences

model:

$$Y_{ist} = \alpha_s + \gamma_t + \beta \cdot \text{Ban}_{st} + X'_{ist}\delta + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is a mental health outcome for student i in state s during survey year t , Ban_{st} indicates whether state s has an effective conversion therapy ban in year t , α_s and γ_t are state and year fixed effects, and X_{ist} includes individual controls (sex, race/ethnicity, and grade). Standard errors are clustered at the state level.

The identifying assumption is parallel trends: absent the ban, mental health outcomes in ban-adopting states would have evolved in parallel with outcomes in non-adopting states. While this assumption is untestable, I provide several pieces of supporting evidence. First, placebo outcomes (bullying at school, electronic bullying) that should not be directly affected by conversion therapy bans show no significant treatment effects. Second, the event study shows no evidence of pre-trends. Third, the concentration of effects among LGB youth—the population the policy targets—provides a built-in placebo, since any state-level confounder coincident with ban adoption would need to differentially affect LGB youth to explain the interaction pattern.

4.2 Heterogeneity by Sexual Identity

In the 2021–2023 subsample with sexual identity data, I estimate:

$$Y_{ist} = \beta_1 \text{Ban}_{st} + \beta_2 \text{LGB}_i + \beta_3 (\text{Ban}_{st} \times \text{LGB}_i) + X'_{ist}\delta + \gamma_t + \varepsilon_{ist} \quad (2)$$

where β_3 captures the differential effect of bans on LGB relative to heterosexual youth. With only two survey waves in the sexual identity subsample, identification relies on cross-state variation in ban status rather than within-state changes. I cluster standard errors at the state level throughout.

4.3 Callaway–Sant’Anna Estimator

As a robustness check, I estimate the group-time average treatment effect using the [Callaway and Sant’Anna \(2021\)](#) estimator, which avoids the negative weighting problem that can arise in TWFE with staggered treatment timing. I use not-yet-treated states as controls with the universal base period.

Table 2: Effect of Conversion Therapy Bans on Adolescent Mental Health

	Persistent Sadness (1)	Considered Suicide (2)	Suicide Plan (3)	Suicide Attempt (4)
<i>Panel A: No individual controls</i>				
Ban enacted	-0.0250*** (0.0078)	-0.0117 (0.0072)	-0.0160* (0.0083)	-0.0048 (0.0042)
<i>Panel B: With individual controls</i>				
Ban enacted	-0.0279*** (0.0082)	-0.0124* (0.0073)	-0.0175** (0.0083)	-0.0065 (0.0046)
Pre-treatment mean	0.319	0.182	0.15	0.096
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Individual controls	No/Yes	No/Yes	No/Yes	No/Yes
Observations			743,593	
States			39	

Notes: Each column reports a separate difference-in-differences regression of the outcome on an indicator for whether the state had enacted a conversion therapy ban. Panel A includes state and year fixed effects only. Panel B adds individual controls (sex, race, and grade). Standard errors clustered at the state level in parentheses. Survey weights applied. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5. Results

5.1 Main Results

Table 2 presents the main difference-in-differences estimates. Panel A reports specifications with state and year fixed effects only; Panel B adds individual controls. Across both panels, the results are qualitatively similar, with controls producing slightly larger estimates.

The preferred specification (Panel B) shows that conversion therapy bans reduce persistent sadness by 2.8 percentage points ($p < 0.01$), representing an 8.7% decline from the pre-treatment mean of 31.9%. Suicide planning declines by 1.8 percentage points ($p < 0.05$), a 11.7% reduction from the 15.0% baseline. The estimate for suicidal ideation is -1.2 percentage points ($p = 0.10$), and for suicide attempts -0.6 percentage points ($p = 0.16$).

The gradient across outcomes—largest effects for the most common outcome (persistent sadness), declining for rarer outcomes (suicide attempts)—is consistent with a destigmatization mechanism that broadly improves emotional well-being rather than narrowly preventing attempts. Survey-based measurement of rarer events is also noisier, contributing to reduced precision for suicide attempts.

Table 3: Heterogeneity by Sexual Identity (2021–2023)

	Persistent Sadness (1)	Considered Suicide (2)	Suicide Plan (3)	Suicide Attempt (4)
<i>Panel A: Interaction specification</i>				
Ban enacted	-0.0356*** (0.0107)	-0.0138** (0.0063)	-0.0185** (0.0090)	-0.0062 (0.0058)
Ban × LGB	-0.0087 (0.0189)	-0.0457*** (0.0146)	-0.0425*** (0.0140)	-0.0393*** (0.0088)
<i>Panel B: LGB subsample</i>				
Ban enacted	-0.0408* (0.0216)	-0.0580*** (0.0193)	-0.0582*** (0.0197)	-0.0454*** (0.0116)
<i>Panel C: Heterosexual subsample</i>				
Ban enacted	-0.0358*** (0.0104)	-0.0139** (0.0061)	-0.0188** (0.0086)	-0.0063 (0.0059)
Year FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Observations (Panel A)			195,230	

Notes: Panel A reports the interaction of ban status with LGB identity, controlling for year fixed effects, sex, race, and grade. Panel B restricts to LGB-identified students; Panel C to heterosexual students. Standard errors clustered at the state level. Sample limited to 2021–2023 waves where the sexual identity question was asked. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Heterogeneity by Sexual Identity

Table 3 presents results decomposed by sexual identity. Panel A reports the interaction specification from Equation (2). The Ban × LGB interaction is significant at the 1% level for three of four outcomes: suicidal ideation (−4.6 pp, $p < 0.01$), suicide planning (−4.3 pp, $p < 0.01$), and suicide attempts (−3.9 pp, $p < 0.001$). For persistent sadness, the interaction is negative but small and insignificant (−0.9 pp), consistent with the broad destigmatization channel affecting both groups similarly for this outcome.

Panels B and C separate the samples. Among LGB youth in ban states, suicide attempts are 4.5 percentage points lower ($p < 0.01$), compared to 0.6 percentage points for heterosexual youth ($p = 0.28$). The LGB effect represents a 19.5% decline from the baseline rate of 23.1% in untreated states. This concentration of effects among the targeted population is the key mechanism test: it is difficult to construct a confounding story that generates large effects for LGB youth but near-zero effects for heterosexual youth within the same states and time periods.

5.3 Robustness

Table 4 reports robustness checks for the population-level DiD.

Event study. I estimate event-study specifications using relative time to treatment for treated states. The pre-treatment coefficients for persistent sadness are small, negative, and statistically insignificant at all leads, consistent with parallel trends. The pattern is similar for suicidal ideation and suicide planning: no evidence of differential pre-trends in the two biennial waves before adoption.

Alternative estimator. The Callaway and Sant’Anna (2021) estimator yields an aggregate ATT of -0.94 percentage points for persistent sadness, which is smaller than the TWFE estimate (-2.8 pp) and statistically insignificant. This attenuation warrants discussion. First, the CS estimator operates on state-year means (39 units \times 5 waves) rather than individual-level data ($743,593$ observations), discarding substantial within-state variation and reducing statistical power. Second, several treatment cohorts contain only 1–2 states, yielding imprecise group-time estimates that attenuate the aggregate. Third, the sign is consistently negative across all four outcomes, suggesting the direction of effects is robust even if the magnitude is sensitive to the estimation approach. The TWFE–CS gap may partly reflect negative weighting in the TWFE specification (Sun and Abraham, 2021), though the consistent sign across estimators provides reassurance.

Dropping early adopters (California, New Jersey), which have at most one pre-treatment survey wave, produces estimates of -2.1 pp for persistent sadness ($p < 0.001$), confirming the result is not driven by these states.

Placebo outcomes provide reassurance against confounding. Bullying at school (-0.9 pp, $p = 0.20$) and electronic bullying (-0.5 pp, $p = 0.33$) show no significant effects, as expected—conversion therapy bans target practitioner behavior, not peer interactions.

Leave-one-out exercises for the persistent sadness outcome show the coefficient ranges from -2.8 to -1.8 pp. California’s removal produces the largest attenuation (to -1.8 pp), consistent with its role as the earliest and largest adopter, but the estimate remains significant.

6. Discussion

The central finding—that conversion therapy bans measurably reduce adolescent mental health distress, with effects concentrated among LGB youth—supports a “destigmatization dividend” interpretation. The direct pipeline through which bans prevent actual conversion therapy is likely small: estimates suggest fewer than 700,000 adults have ever undergone

Table 4: Robustness Checks

	Persistent Sadness	Considered Suicide	Suicide Plan	Suicide Attempt
<i>A. Baseline (from Table 2, Panel B)</i>				
Ban enacted	-0.0279*** (0.0082)	-0.0124* (0.0073)	-0.0175** (0.0083)	-0.0065 (0.0046)
<i>B. Callaway–Sant’Anna</i>				
ATT	-0.0094	-0.0020	-0.0048	-0.0016
<i>C. Drop early adopters (CA, NJ)</i>				
Ban enacted	-0.0208*** (0.0057)	-0.0065 (0.0049)	-0.0109* (0.0056)	-0.0060 (0.0046)
<i>D. Placebo outcomes</i>				
Bullied at school			-0.0087 (0.0067)	
Electronic bullying			-0.0052 (0.0053)	

Notes: Panel A reproduces the baseline specification from Table 2. Panel B reports the aggregate ATT from the Callaway and Sant’Anna (2021) estimator using not-yet-treated states as controls. Panel C drops California and New Jersey (adopted bans in 2012–2013, with limited pre-treatment data). Panel D tests placebo outcomes that should not be directly affected by conversion therapy bans. All specifications include state and year fixed effects, individual controls, and state-clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

conversion therapy in the U.S. (Blosnich et al., 2020; Conron et al., 2020), and the share of minors currently receiving it in any given year is far smaller. The larger mechanism operates through signaling and norm-shifting.

When a state legislature enacts a conversion therapy ban, it makes three implicit statements: that homosexuality is not a disorder requiring treatment, that attempts to change sexual orientation are harmful, and that LGB minors deserve legal protection. This normative signal can affect youth mental health through multiple pathways. It may reduce felt stigma among sexual minority youth (Hatzenbuehler et al., 2014), shift the behavior of families considering conversion therapy, alter the clinical default among practitioners, and contribute to a broader environment in which LGB identity is affirmed rather than pathologized.

The fact that persistent sadness declines for all adolescents, not just LGB youth, is consistent with this interpretation. Anti-discrimination signals can improve social climate broadly—reducing hostile environments that affect all students—while concentrated benefits accrue to the targeted group. Raifman et al. (2017) document a similar pattern for same-sex marriage legalization, finding population-level improvements in adolescent mental health

concentrated among sexual minorities.

The magnitude of the estimates is policy-relevant. The 4.5 percentage point reduction in LGB suicide attempts, applied to the approximately 2.4 million LGB high school students nationwide, implies roughly 108,000 fewer suicide attempts per year among this population—a first-order public health effect. Even accounting for the imprecision of this back-of-envelope calculation and the cross-sectional nature of the sexual identity analysis, the implied benefits are large relative to the minimal implementation costs of these laws.

Limitations. Four caveats deserve emphasis. First, the sexual identity analysis relies on cross-state comparisons in 2021–2023, not within-state pre-post variation, since the YRBSS sexual identity question is available only in these waves. Any unobserved state-level factor that coincides with ban adoption and differentially affects LGB youth—such as targeted school support programs or local advocacy—could bias the heterogeneity estimates. Second, conversion therapy bans correlate with broader pro-LGBTQ+ policy environments, including non-discrimination laws and anti-bullying statutes. While the concentration of effects among LGB youth provides a powerful placebo-like test (state-level confounders would need to differentially affect LGB youth specifically), I cannot fully rule out that the ban indicator proxies for a bundle of correlated policies. Third, the YRBSS is a school-based survey and excludes dropout, homeschooled, and institutionalized youth, who may be disproportionately affected by both conversion therapy and mental health distress. Fourth, I cannot distinguish between the direct effect of preventing conversion therapy and the indirect signaling effect; the large LGB-specific effects relative to the likely thin direct pipeline suggest signaling dominates, but I cannot estimate each channel separately.

7. Conclusion

State conversion therapy bans—laws that prohibit a small set of clinical practitioners from performing a specific procedure on minors—generate measurable improvements in adolescent mental health at the population level. The effects are large and precisely estimated for persistent sadness and suicide planning, and they concentrate dramatically among LGB youth for suicidality outcomes. This pattern is consistent with a destigmatization dividend: the primary benefit of these bans is not the prevention of a rare practice, but the normative signal that accompanies legislative action.

For the 28 states without conversion therapy bans, the evidence suggests that adoption would produce meaningful reductions in adolescent mental health distress at effectively zero fiscal cost.

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Project Repository: <https://github.com/SocialCatalystLab/ape-papers>

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A. Data Appendix

YRBSS data construction. The Youth Risk Behavior Survey State and District Combined (SADC) dataset is downloaded from the CDC website as fixed-width ASCII files, with variable positions defined by the 2023 SPSS syntax file. I extract demographic variables (age, sex, race, grade), sexual identity, survey weights, and mental health outcomes (questions Q26–Q29, recoded as binary indicators QN26–QN29). States are identified by the two-character site code. The analysis sample includes all state-level observations from the 2015–2023 waves.

Treatment coding. Conversion therapy ban effective dates are hand-coded from the Movement Advancement Project database and verified against state legislative records. The 22 states with bans enacted by the end of 2023 are: California (2012), New Jersey (2013), District of Columbia (2014), Oregon (2015), Illinois (2016), Vermont (2016), Connecticut (2017), Nevada (2017), New Mexico (2017), Rhode Island (2017), Delaware (2018), Hawaii (2018), Maryland (2018), New Hampshire (2018), Washington (2018), Colorado (2019), Maine (2019), Massachusetts (2019), New York (2019), Utah (2020), Virginia (2020), and Minnesota (2023). Of these, 14 appear in the YRBSS analysis sample.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled (all students, 2015–2023)</i>						
Persistent sadness	-0.0279	0.0082	0.466	-0.060	0.018	Moderate negative
Considered suicide	-0.0124	0.0073	0.386	-0.032	0.019	Small negative
Suicide plan	-0.0175	0.0083	0.357	-0.049	0.023	Small negative
Suicide attempt	-0.0065	0.0046	0.294	-0.022	0.016	Small negative
<i>Panel B: Heterogeneous (by sexual identity, 2021–2023)</i>						
Suicide attempt (LGB)	-0.0454	0.0116	0.422	-0.108	0.028	Moderate negative
Suicide attempt (Heterosexual)	-0.0063	0.0059	0.254	-0.025	0.023	Small negative

- **Notes:** **Country:** United States. **Research question:** Do state laws banning licensed professionals from performing conversion therapy (SOGIECE) on minors reduce adolescent mental health distress? **Policy mechanism:** Conversion therapy bans prohibit licensed mental health professionals from attempting to change a minor’s sexual orientation or gender identity, directly protecting youth from a harmful practice and signaling legislative recognition that such practices are damaging. **Outcome definition:** Binary indicators from the CDC Youth Risk Behavior Survey for persistent sadness/hopelessness (felt sad or hopeless almost every day for two or more weeks in a row during the past 12 months), suicidal ideation (seriously considered attempting suicide), suicide planning (made a plan about how they would attempt suicide), and suicide attempts (actually attempted suicide on one or more occasions). **Treatment:** Binary; equals one if the student’s state had enacted a conversion therapy ban effective before or during the YRBSS survey year. **Data:** CDC Youth Risk Behavior Survey (YRBSS) State and District Combined (SADC) datasets, 2015–2023 (five biennial waves), individual student-level observations, $N = 743,593$ across 39 states. **Method:** Two-way fixed effects difference-in-differences with state and year fixed effects, individual controls (sex, race, grade), standard errors clustered at the state level, survey weights applied. **Sample:** High school students (grades 9–12) in states participating in the YRBSS; Panel B restricts to 2021–2023 waves where the sexual identity question was administered. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).